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INAUGURAL ESSAY
ON THE
IMPULSE AND SOUNDS
ATTENDING THE
ACTION OF THE HEART IN ITS NORMAL
STATE;

SUBMITTED TO THE
Medical Faculty of the University of
Edinburgh,

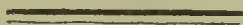
IN CONFORMITY WITH THE RULES FOR GRADUATION,

BY AUTHORITY OF THE
VERY REVEREND PRINCIPAL BAIRD,

AND WITH THE SANCTION OF THE
SENATUS ACADEMICUS.

BY
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CANDIDATE
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THE FOLLOWING ESSAY IS ENCLOSED

TESTIMONY OF RESPECT FOR HIS EXCELLENT EDUCATION

AND LITERARY TALENT

AND OF GRATITUDE FOR HIS KIND AND GENEROUS

67

THE AUTHOR

G. Child Esq.

with

The Authors sincere regards

TO

J. H. DAVIDSON, M. D.

F. R. S. E.,

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH,

&c. &c. &c.

THE FOLLOWING ESSAY IS INSCRIBED,

IN TESTIMONY OF RESPECT FOR HIS EMINENT PROFESSIONAL

AND LITERARY TALENTS,

AND OF GRATITUDE FOR HIS PRIVATE FRIENDSHIP,

BY

THE AUTHOR.

TO

WILLIAM PULTENEY ALISON, M.D.,

F. R. S. E.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, AND PROFESSOR
OF THE INSTITUTES OF MEDICINE IN THE UNIVERSITY
OF EDINBURGH;

THIS ESSAY IS GRATEFULLY INSCRIBED,

AS A TRIBUTE OF RESPECT AND ADMIRATION,

BY

THE AUTHOR.

ON THE

IMPULSE AND SOUNDS

ATTENDING THE

ACTION OF THE HEART IN ITS NORMAL
STATE.

WHEN we reflect on the important part which the Heart performs in the animal economy, and the extensive influence which any irregularity in its action necessarily exercises on the rest of the system, we must at once admit how essential it is, that our means of immediately recognising such irregularities be as little fallible as possible ; and as it is obviously impossible to understand the diseased action of any organ, without a thorough acquaintance with all the phenomena of its actions in the normal state, it cannot be denied that an accurate knowledge of the cause of the Impulse and Sounds which attend the healthy

action of the heart, is of paramount importance as well to the practical man as to the physiologist. Nevertheless, there are few subjects on which the minds of professional men have been more divided than on this; and it is still a question much agitated, not only what are the immediate causes of this impulse and sounds, but even during what periods of the heart's action each of them respectively takes place.

It is this discrepancy among physiologists, in conjunction with the extreme interest of the question, which has particularly invited my attention to the subject; in discussing which, I propose to speak, first of the Impulse, and secondly of the Sounds which attend the healthy action of the heart, and, under each head, to give a rapid sketch of the principal hypotheses which have been at various times broached to explain them, and of the chief objections to which each appears to be liable, preparatory to adopting the explanation which seems, in the present state of our knowledge, to be the most satisfactory.

And, *first*, With respect to the Impulse which attends the action of the Heart. That

this takes place during the more prolonged of the two sounds which are heard on applying the ear to the præcordial region, and of which I am to speak more particularly in future, is undeniable; but it has been questioned, first, Whether it is felt during the systole or diastole of the ventricles? and, secondly, assuming this question to be decided, What is its immediate or proximate cause? By all the earlier authors, and indeed by all the more modern also, with the exception of Drs Corrigan* and Stokes†, and their adherents, to whose opinions I shall presently have to allude, it has been generally admitted that the impulse of the heart corresponds with the systole of the ventricles, (an opinion founded on the certainly apparent, and probably real synchronism of the impulse at the chest, with the pulse at the arteries); but how it happens that the heart during this systole of its ventricles should be forcibly propelled against the parietes of the chest, remained to be explained.

* Dublin Medical Transactions, vol. i. p. i.; also *Lancet*, 1829.

† Edin. Med. & Surg. vol. xxxiv. p. 269.

By Vesalius, Riolan, Borelli and Winslow,* it was supposed that the impulse depended on the elongation of the organ, owing to its contraction, since they could not conceive that if the heart were shortened, it would not rather recede from the ribs than approach them. This hypothesis was, however, overturned by Bassuelle, who demonstrated that if the ventricles were elongated during the action, the auriculo-ventricular valves would be opened instead of closed, and the co-operation of the columnæ carneæ, instead of obviating as they do the bad consequences of the shortening of the ventricles, would have only increased the mischief. The course of the principal muscular fibres of the ventricles also running as they do in a spiral manner round their cavities, is obviously incompatible with any elongation of them during their contraction, and must tend as certainly to draw the apex of the heart towards its fundus, as that of the fibres of the intercostal muscles to draw one rib towards the other. Nay, it is susceptible of ocular demonstration, that when the heart contracts, the organ is not elon-

* Winslow, *Anatomy of the Human Body*, Trans. by Douglas.

gated, but, on the contrary, drawn up upon its base.

By Haller,* Senac,† and others, who also conceived that the impulse occurred during the systole of the ventricles, it was referred to the rapid filling of the auricles at the moment when the ventricles contract; so that the former not being able from their situation to expand towards the spine, necessarily press in their dilatation, the heart against the ribs. This hypothesis is, however, easily set aside; for, in the first place, the blood enters the auricles not suddenly, but gradually; and, secondly, it has been most satisfactorily shewn by Hope,‡ that the impulse has taken place without the auricles acting at all.

A third opinion is that of Hunter, Bostock,§ and others, who, agreeing with the preceding authors in believing that the impetus occurs during the systole of the ventricles, attribute it to the tendency in the arch of the aorta to assume the straight direction, while the new cur-

* *Elementa Physiologiæ.*

† Senac, *Traité de la Structure du Cœur.*

‡ Hope on *Diseases of the Heart.*

§ Bostock's *Physiology.*

rent of blood is passing through it; and this great vessel being the fixed point, while the heart, as its appendage, hangs loose and pendulous, the influence of its own action is thrown back upon itself, and it is thus tilted forwards against the inside of the chest. But to waive the obvious fallacy of arguing, as Hunter does in this instance, respecting the actions of the human heart from the phenomena displayed by that of the lower animals, the situation and relations of which are often so different, it is clear that if his explanation were true, we should find the heart tilted, not against the left side of the chest, but against the right, in consequence of following the direction of the straightening of the aorta. Besides, it is evident that here the effect is made anterior to the cause; for the new wave of blood cannot be sent into the aorta before the heart has contracted, and is consequently again filling: so that to ascribe the beat of the heart to this cause, and at the same time affirm that it takes place during the systole of the ventricles, is obviously incorrect. According to Sabatier, when the ventricles contract, a portion of blood is forced behind the valves, and in this way the heart is pushed for-

wards ; but it is hardly necessary to remark, that this circumstance is quite inadequate to explain the violent impulse of the heart, the portion of blood thus thrown behind the valves being much too small for the purpose.

Besides, it is obvious that an impulse exerted within a body cannot produce any effect on its component parts. If, for example, a person placed in the stern of a boat, press against the prow, no result, no motion is produced, as in that case the person, the instruments with which he pushes, and the boat, all form one collective body. In the same way the parietes of the ventricle, the contained fluid, and the valves, are merely several parts of one general mass, and the action, therefore, of one part upon another must be equally nugatory.

It is almost unnecessary to state the opinion of Dr Barry on this subject, by whom the impulse of the heart is attributed to the rushing forward of the whole organ, in order to fill the vacuum formed by the drawing inwards of its apex, since an obvious objection to this is offered by the fact that the heart at all times contains nearly the same quantity of blood, and that the actual space occupied by a muscle is

the same, whether this muscle be at rest or in action, so that the bulk of the heart remaining the same during the systole as during the diastole of its ventricles, no approach to a vacuum can ever occur.

We come next to the doctrine of Mayo*, which has been adopted by many of the physiologists of the present time. According to this doctrine, during the systole of the ventricles, the apex of the heart is tilted forwards against the chest, owing to the fact that a greater number of fibres goes to the anterior part of the heart than to the posterior, so that when they are all called into action, the anterior overcome the others, and they produce a curvature of the apex, bringing it into sudden and violent contact with the parietes of the chest. This doctrine, however, is founded on the presumption, that the phenomena displayed by the heart, when acting out of the body, are the same as those which occur while it is in the body, and acting upon its own blood—a presumption which is certainly too hasty. It has been demonstrated lately, and was particularly insisted upon in a paper read the season before last in

* Mayo's Physiology.

the Medical Society of Edinburgh by Dr Waterhouse, that the various sets of fibres constituting the parietes of the left ventricle, all those which descended as far as the apex of the heart in their return towards the fundus, formed the *columnæ carneæ*, while the rest constituted, some the *septum ventriculorum*, and others the walls of the right ventricle. Now, it follows, that when the heart is removed from the body and empty, the valves not being raised during the contraction of the ventricle, nor the *columnæ carneæ* therefore put upon the stretch, the apex no longer pulled equally from within as from without, must really be tilted forwards; but this cannot occur, at least to the extent supposed by Mayo, when the heart is acting upon its natural contents, and when, consequently, the valves being raised, its apex is pulled almost equally in both directions.

So far we have proceeded upon the supposition that the impulse of the heart takes place during the systole of the ventricles, however different have been the several explanations of its immediate cause.

In their observations, Drs Corrigan and Stokes take up an entirely different view from

those already cited, and their remarks have accordingly produced considerable interest among scientific men. These gentlemen are desirous of shewing that the impulse of the heart occurs not during the systole, but during the diastole of the ventricles, and that it arises from the distention and consequent elevation of this organ, and not from the tilting forwards of the apex of the heart, or any other cause previously assigned.

In order to establish the justness of this hypothesis, it is essential to prove that the impulse of the heart is not, as has been hitherto presumed, synchronous with the pulse of the arteries, but that a very appreciable interval takes place between the two ; and this has accordingly been confidently stated as the case by Corrigan, more particularly with respect to the radial artery. He brings forward, amongst other arguments, a pathological fact in favour of this doctrine, viz. a case of narrowing of the auriculo-ventricular opening, in which he states that the *Bruit de Soufflet* was most distinctly heard anterior to the pulse, and during the impulse against the chest. This he considers as one strong argument in favour of his opinion,

that the impulse of the heart is caused, not by the systole, but by the diastole of the ventricles, and he adduces farther many experiments and observations in support of the same doctrine. For example, on observing the heart of a lower animal, after the ribs were removed, it was evident to Dr Corrigan and others, that at each contraction of the auricles, corresponding of course with the diastole of the ventricles, the heart came forward, but retired upon the ventricles contracting. He could observe no tilting forwards of the heart, but merely an application of the organ to the chest in the latter case; and Corrigan says, that, after examining the heart of a frog, he had not a doubt upon the facts he had established by experiment upon warm-blooded animals. He conceives that he made out by his experiments that the auricles contract first, then the ventricles, and that the contraction of the latter is succeeded by a state of rest: that the contraction of the ventricles is rapid, and follows quick as can be conceived after that of the auricles, the contraction of which is comparatively slow; and that the heart strikes the side, or gives the impulse, when the auricles contract, while it retires from

the side during the contraction of the ventricles, the beat of the heart being produced not by a tilting up of the point of the organ as previously supposed, but by its swelling and coming against the ribs, in consequence of the impulse given by the rush of blood from the auricles. Dr Corrigan states that the more frequent the action of the heart is, the shorter of course is the interval between the impulse at the chest, and the pulse in the radial artery; and hence has arisen the mistake of supposing that they were synchronous.

But I must now inquire a little into the justness of their conclusions. And, in the first place, it may be remarked, that it has been unquestioned by all physiologists, from the earliest period to the present time, with the exception of the gentlemen whose views I am now engaged in discussing, that the impulse of the chest and pulse of the arteries are synchronous; and I am much inclined to think that those who agree with Dr Corrigan in the opposite opinion, have not investigated the facts quite so minutely as they should have done, previously to presenting so novel a doctrine upon the subject.

It is, as I have before remarked, evident, that in order to render the opinion, that the impulse of the heart is occasioned by the diastole of the ventricles satisfactory, the fact must be established of the non-synchronism of the pulse of the arteries with the impulse felt at the side; for, according to Corrigan's views, these two phenomena are produced by different and successive actions of the heart, and therefore cannot occur simultaneously. But I conceive that the slight interval which elapses between the impulse of the chest and the pulse of the radial artery, can be perfectly well explained upon the assumption of the progressive motion of the blood. If we place one hand on the præcordial region, and apply the finger of the other on the artery of the wrist, we shall undoubtedly find that there is an interval, varying according to the rapidity of the pulse; but if, instead of applying one hand to the wrist, we do so to the carotid or subclavian, it will immediately be evident that the impulse of the heart and pulse in these arteries are synchronous; and I must therefore conclude that the very short period which elapses between the impulse at the chest and the pulse of

the radial artery, is attributable to the distance which the impulse has to travel, more especially as this period is found to be longer the more distant from the heart the artery is situated.

I met with a case last autumn, which confirmed the position I am desirous of establishing, better than any experiment that could be performed. From a disease, of which it is unnecessary to make mention at present, the pulse was so low as twenty-eight in a minute, and regular ; the rhythm also seemed to me to be natural. It was in this case quite evident on applying the hand at once to the chest and to the subclavian artery, that the pulsations were simultaneous ; but on feeling the radial artery, there was a perceptible difference, which was still greater at the ankle. There could not be in this case the deception which Corrigan mentions, "that in warm-blooded animals the heart's action is so rapid that the lapse of time between the impulse at the chest and the beat of the radial artery is almost imperceptible," since the pulse here was so slow that any such interval would have been easily perceived. I may state, that this case presented itself after I had perused Corrigan's paper on the subject ; and

at a time when I considered that there could not be two opinions as to the justness of his views; but it appeared so clear to me, that the impulse at the chest corresponded with the pulse of the contiguous arteries in this case, that I resolved on following out the inquiry. This case was visited along with me by a gentleman, whom I requested to examine the various phenomena of the heart's action, without mentioning my own opinion, and he stated that he found the impulse at the chest and the pulse at the subclavian artery entirely simultaneous, but that there was a slight interval between the former and the pulse at the wrist.

There is, besides, a strong argument against Corrigan's views, in an experiment performed by Hope, where the impulse at the chest occurred regularly for a pulsation or two, when the auricles were stationary.

In a paper to which I have already alluded, presented to the Medical Society a short time ago, the author, who was a proselyte to Dr Corrigan's views, seemed to lay considerable stress upon a fact which I believe few doubt, "that the diastole of a vigorous heart is sufficient to push up the compressing hand."

Richerand, Williams, and others state, that the resiliency of the heart, supposed by Bichat to be a vital action, as well as its contraction, is so strong as to raise the hand compressing it, and this is described as taking place after the contraction of the ventricle, owing to a power inherent in itself, and not depending upon its dilatation by the blood; but whether it is during this dilatation of the ventricles that the impulse of the heart is perceived, is very problematical. If this were the case, how could we explain the synchronism of the pulse, which I have already, I trust, proved; and how could it happen that the impulse does not begin with the time of the heart's repose, during which the ventricles are in a state of at least passive diastole?

Dr Billing conceives that if the evidence can be depended upon, that the columnæ carneæ are a continuation of the fibres of the apex, it necessarily follows that when the heart contracts upon its contents, the valves are thrown up by the forcible pressure of the blood against them, and the columnæ carneæ now contracting, in order to prevent the valves from falling into the auricles, must draw up the apex, and

thus give a more rounded shape to the heart. But the heart, situated as it is with respect to the ribs and diaphragm, cannot assume this more rounded form, but it must operate almost in the manner of a wedge upon the parts in question. He feels persuaded that, upon this wedge-like operation, the impulse of the chest depends, and consequently it is, as first presumed, during the systole, and not during the diastole of the ventricles, that this impulse occurs.

Although, at first sight, this opinion appears plausible, yet I cannot understand how it can account fully for the phenomena in question, because it would require first to be proved, from the anatomical structure and arrangement of the parts, that the heart has not room to enlarge laterally ; and, allowing that this was explicable, it would then be necessary to shew what effect the obstacles to the lateral dilatation of the organ, and the drawing of the apex upwards to the base of the heart, would have upon its action, and by what mechanical law it would throw its apex against the chest.

From the review which I have made of the various opinions of authors concerning the

causes of the impulse of the heart, and considering the objections which have been stated to them, I am inclined, in the present state of the question, to conclude that the doctrine of Mayo is the one least liable to fallacy, although, from what has been stated, I trust I have shewn that it can only be adopted in default of one less liable to objection.

I now turn to the consideration of the causes of the Sounds which attend the action of the heart; but in respect to this question I need scarcely say more than that if the position which I have endeavoured to maintain in this paper, that the impulse against the chest is caused by the systole, and not by the diastole, of the ventricles be true, it is a necessary consequence that the explanation of the sounds must be in correspondence with it. It is sufficiently well known that upon applying the ear, particularly if assisted by the stethoscope, to the chest during the action of the heart, there is heard, first, a dull and prolonged; and then an acute and sharp sound, which is succeeded by a short interval of silence. After this the dull prolonged sound is repeated, and the same series of phenomena,

constituting what is called the Rhythm of the heart, is incessantly renewed. This dull and prolonged sound is uniformly synchronous with the impulse of the heart ; consequently, if my previous conclusions be correct, must be attendant on the contraction of the ventricles.

It may not be improper, however, to give a summary of Corrigan's views of the subject.—

According to him, the long sound corresponding to the impulse comes first, next the pulse of the arteries, and then the second or short sound. This first sound is, of course, ascribed to the slow dilatation of the ventricles ; their contractions are presumed to give rise to no sound, but to be synchronous with the pulse of the arteries ; while the second sound is said to arise from the sides of the ventricles flapping against each other. Upon this opinion, after what has been already said, it is superfluous to dwell.

Laennec* conceived that the first sound was caused by the contraction of the ventricles ; in which opinion all those who hold that the impulse of the chest and pulse of the arteries are synchronous, coincide ; but he was obviously incorrect in his explanation of the cause of the

* Laennec on Diseases of the Chest, by Forbes.

second sound, which he attributed to the contraction of the auricles. It has been clearly shewn by Professor Turner,* that to make the contraction of the auricles succeed, instead of precede, that of the ventricles, and the state of repose follow the former instead of the latter, is irreconcilable with every known fact regarding the circulation of the blood ; but it is unnecessary to go on to prove this, since I believe no one now holds a similar opinion.

Professor Turner's conjecture on the second sound is, that it depends principally upon the falling back of the heart on the pericardium during its diastole, in the same manner as a compressed bag falls back upon the pressure being removed ; but it appears to be inconsistent with this explanation, that the pericardium is not a stiff bag enclosing the heart and remaining always stationary, but a flexible membrane, if not following every where the movements of its containing organ, at least partaking in them to a considerable extent. Besides, it is stated by authors† that the sound continues when the heart pulsates out of the pericardium.

* Med. Chir. Transactions.

† Williams in Med. Chir. Review.

It is the opinion of M. Magendie*, that it is the ventricular diastole projecting the apex of the heart against the chest, which is the cause of the Impulse and first Sound. From what I have stated in the former part of this paper, when objecting to the opinion that the Impulse occurred during the diastole, I consider it quite unnecessary to dwell longer on it here, hoping that I have proved that such does not take place. As to that physiologist's theory of the formation of the first sound depending upon the concussion which the chest receives from the heart, I believe that what has already been mentioned as militating against Professor Turner's cause of the second sound, will be a sufficient argument against this. The cause to which he refers the formation of the second sound, is consistent with that of the first, but I think equally fallacious. He states that it depends upon the systole of the heart impelling its base against the walls of the chest. In addition to the objection which has been brought forward as to the cause of the first sound, which necessarily avails here

* Lecture read before the College of France, quoted by M. Pigeaux.

also, I would mention that such an opinion is refuted by the fact already taken notice of, that the second sound occurs distinctly after the pulse in the large arteries, and therefore synchronous with the diastole, not the systole of the heart.

It may well be a matter of surprise that a physiologist of M. Magendie's experimental eminence, should have so much overlooked the value of such investigation on the present subject, and I may add, the distinct statements of Hope and others.

Dr Williams* asserts that the second sound depends on the diastole of the ventricles, which he thus explains. During the systole of the ventricles, the columnæ carneæ, which hold the valves, are stretched to their full extent, but as soon as the ventricles cease their contraction, these muscles act with energy, and bring the valves with such a sudden "slap" against the side of the ventricles, as to produce the sound in question. But this theory cannot, I conceive, explain the subject in dispute, for the slightest inspection of the anatomy of the heart, will at once shew that, even allowing that the

* North of England Journal, No. IV.

muscles did pull back the valves on the cessation of the systole of the ventricles, they would not strike the side of the cavity, but be brought merely to a line parallel with their sides.

The fact appears to be, that it is not after the contraction of the ventricles, but during their contraction, that the columnæ carneæ act, not only because they are formed by a continuation of the same muscular fibres, but because their action is necessary when the ventricles contract in order to prevent the alæ of the valves from being propelled into the auricles by the pressure of the blood. Dr Williams attempted to prove that they were useless in that capacity, by filling the ventricles of a dead heart from the large arteries, and finding that the valves, instead of being propelled into the auricles, became accurately closed; but the ventricles were not shortened in this experiment, as they are when blood is pressed against the valves during life. The columnæ carneæ do not, as it appears to me, either open the valves, as asserted by Dr Williams, or close them, as asserted by some of his opponents. These valves are both opened and closed by the blood, but they are *kept closed*

when the ventricles are shortened, as they are during their contraction, by the columnæ carneæ, which therefore must act simultaneously with these cavities.

I must now direct my attention to the views of Dr Hope, which, from their simplicity, and being so admirably corroborated by experiment, I am much inclined to follow. He states, that, on opening the chest of a living animal, there is observed first the auricular contraction which runs into that of the ventricles, simultaneously with which occurs the drawing up of the apex of the heart towards the base, the bulk of the heart, and particularly the apex, being at the same time slightly thrown forward: the pulse of the contiguous arteries, he asserts, is synchronous with this impulse and first sound,—a position which I have all along maintained. Dr Hope coincides with most physiologists in supposing that the first sound is caused by the expulsion of the blood from the ventricles, and the consequent collision of the particles of fluid which necessarily takes place. However ingenious this theory may appear, yet Dr Hope has advanced no arguments or facts in direct proof of his hypothesis.

It is a well known and I believe generally received fact, that liquids are much more difficult to excite to sonorous vibrations than other bodies, and hence hesitation must be felt before adopting such an opinion. He has some difficulty in accounting for the second sound, which is loud, brief, and clear. He is inclined, however, to consider that it depends on a passive flow of a portion of blood from the auricles into the ventricles immediately consequent on the diastole of the latter ; that, for an instant after this, neither auricles nor ventricles are contracting, this interval, together with the subsequent contraction of the auricles, during which occurs no sound, constituting the period of silence in the heart's rhythm, and that it is during the continuous contraction of the ventricles alone, as I have stated, that the first or dull sound occurs.

An opinion has been lately broached, that the sounds of the heart can be explained on the fact, first ascertained by Wollaston, that a muscle in contracting gives out sound ; but as this view leaves out of the question all influence from the contained fluid, it would require to be proved that the heart could emit

sound when empty, and not acting on its blood.

Amongst so many different and conflicting opinions proposed and advocated by authors of high reputation, it will not be a matter of surprise that I should feel no small difficulty in coming to a conclusion upon this subject. I feel inclined to suppose, that the blood is a great agent in the production of the sounds, not altogether in itself, or from "collision of its particles," as Dr Hope expresses it; but that the rapid flow coming in contact with the columnæ carneæ, valves, &c., must produce some sound probably sufficient to account for those in question, is evident. This view of the subject, in as far as accounting for the production of the second sound, has the advantage of explaining Laennec's signs of disease of the auricular valves, an object of considerable pathological importance. As to the sound which a muscle in action emits, I cannot consider that as being sufficient to cause the very audible sound which is heard on applying the ear to the præcordial region; although I make little doubt but this may to a certain extent assist, in combination with what has been already stated in reference

to the sound made by the blood leaving its cavities, in producing the sounds.

In conclusion, it may be stated, that I am induced to consider the phenomena of the heart's action as follows. First occurs the auricular contraction without emitting any sound ; this runs into that of the ventricles ; synchronous with which are the impulse at the chest, the dull sound, and the pulse of the great arteries : next the diastole of the ventricles simultaneously with the acute sound ; and, lastly, the interval of repose, unattended of course, as well as the systole of the auricles, with any sound at all. My reasons for adopting this rationale I have already stated. According to the best estimates agreeing with this explanation, it is calculated that the ventricular systole occupies one-half of the time taken up by the whole series of phenomena, and the ventricular diastole one-fourth ; the interval of repose, together with the contraction of the auricles, occupying the remaining fourth.

It would be a subject of the deepest interest to prosecute this explanation of the phenomena attending the action of the heart in its normal

state, through all those which accompany its diseased action, and to observe how it was corroborated or shaken by this review. To do this, however, constitutes no part of the plan of the present Essay, and would be quite incompatible with the limits which it imposes upon me. I may observe, however, that I know of no phenomena attending the anormal action of the heart, which are irreconcilable with, while many are decidedly favourable to, the explanation of the normal action above adopted.

